7th March, 1958.

My dear Dr Macdonald,

I was hoping to hear from you and possibly even to see you before I left the States, where of course I had a very full programme and could not take advantage of the proposal to visit California, which seemed indeed, in comparison with others, a little tepid and unattractive.

I had heard of the claim for damages, which will inevitably be for an enormous sum, and I presume anything may come out of such an action in your press-dominated country. I hope and think it would have no chance of success here.

Seven or eight years ago, when Hill and Doll’s first enquiry came to my knowledge, I got Hill as my guest to the Royal Society Dining Club to explain his findings, and at that time it was clear that he did not wish to claim that the statistical associations observed demonstrated a causative influence. If I understand aright, E. B. Wilson ten or fifteen years earlier had observed the same association, and from what you now say I presume that he had recognized it as inconclusive and probably unimportant for public health. I have always felt that Wilson was outstandingly the best head in the field of American statistics, though I was very glad to see that Joseph Berkson also had not hesitated to condemn the over-confidence with which Hill and Doll’s, and later Hammond’s enquiries had been interpreted.
My concern of course arose from the fact that as a statistical worker since 1915 I have had wide opportunities of observing the fallacies into which people no more experienced than Doll and Hill can fall in the interpretation of statistical data. But more particularly it had early been impressed on my mind that these difficulties of interpretation could only be overcome by the competent design of experiments or sample surveys. This in all cases involves randomization of treatments to material, and it was manifest that in these lung cancer enquiries no randomization had been, or could have been, accomplished. They failed, therefore, in mere logic to establish the nexus or to exclude the obvious possibilities that the association was due to other causes.

Among these, one which must first strike the mind of any geneticist is the possibility that the smoking classes are not genetically equivalent. Over many years, and with the expenditure of much public and semi-public money on cancer research, no attempt seems to have been made to examine this possibility. Moreover, the matter appears to be well within the scope of competent work in human genetics, if that subject had not been studiously ignored by the employees of the Medical Research Council, who, like most medically trained persons I am afraid, have had no opportunity of acquainting themselves with genetical science.

Logically, however, the case seems to fail for sheer lack of evidence of causation. Nevertheless I do think that those responsible for these enquiries, and for the expenditure of money which should be applied to reducing the incidence of cancer, are to be blamed for allowing their work to be made the basis of a
terrifying propaganda, and for neglecting to obtain consultation and collaboration with the other biological sciences.

Of course I should be glad if you cared to use this letter in any way you please.

Sincerely yours,